II. *The Bakerian Lecture for* 1809. *On some new Electrochemical Researches, on various Objects, particularly the metallic Bodies, from the Alkalies, and Earths, and on some Combinations of Hydrogene.* By Humphry Davy, *Esq. Sec.R.S. F.R.S.E. M.R.I.A.*

Read November 16, 1809.

I. *Introduction.*

I HAVE employed no inconsiderable portion of the time that has elapsed, since the last session of the Royal Society, in pursuing the train of experimental enquiries, on the application of Electricity to Chemistry, the commencement and progress of which, this learned Body has done me the honour to publish in their Transactions.

In this communication I shall, as formerly, state the results. I hope they will be found to lead to some views, and applications, not unconnected with the objects of the Bakerian lecture : and though many of them are far from having attained that precision, and distinctness, which I could wish, yet still I flatter myself, that they will afford elucidations of some important and abstruse departments of chemistry, and tend to assist the progress of philosophical truth.

II. *Some new Experiments on the Metals from the fixed Alkalies.*

IN the paper, in which I first made known potassium and sodium to the Royal Society, I ventured to consider these

bodies according to the present state of our knowledge, as undecompounded, and potash and soda as metallic oxides, capable of being decomposed and recomposed, like other bodies of this class, and with similar phænomena.

Since that time, various repetitions of the most obvious of the experiments on this subject, have been made in different parts of Europe. The generality of enlightened chemists, have expressed themselves satisfied both with the experiments, and the conclusions drawn from them : but as usually happens in a state of activity in science, and when the objects of enquiry are new, and removed from the common order of facts, some enquirers have given hypothetical explanations of the phænomena, different from those I adopted.

MM. Gay Lussac and Thenard, as I have mentioned on a former occasion, suppose potassium and sodium to be compounds of potash and soda, with hydrogene; a similar opinion seems to be entertained by M. Ritter. M. Curadau* affects to consider them as combinations of charcoal, or of charcoal and hydrogene, with the alkalies; and an Enquirer † in our own country, regards them as composed of *oxygene* and *hydrogene*.

I shall examine such of those notions only, as have been connected with experiments, and I shall not occupy the time of the Society with any criticisms on matters of mere speculation.

In my two last communications, I have given an account of various experiments on the action of potassium upon ammonia, the process from which MM. Gay Lussac and Thenard, derive their inferences. At the time that these papers were

* Journal de Physique June, 1808.   † Nicholson's Journal. August, 1809. p. 258.

written, I had seen no other account of the experiments of the French chemists, than one given in a number of the Moniteur, and as this was merely a sketch, which I conceived might be imperfect, I did not enter into a minute examination of it. I have since seen a detail of their enquiry in the second volume of the Mem. d'Arcueil, a copy of which M. BER-THOLLET has had the goodness to send me, and the publication of which is dated June 7, 1809 : and from this detail, it seems that they still retain their opinion ; but upon precisely the same grounds as those to which I have before referred. That no step of the discussion may be lost to the Society, I shall venture to state fully their method of operation, and of reasoning.

They say that they heated potassium* in ammonia, and they found that a considerable quantity of ammonia was absorbed ; and hydrogene produced ; and that the potassium became converted into an olive coloured fusible substance ; by heating this substance strongly, they obtained three fifths of the ammonia again, two fifths as ammonia, one fifth as hydrogene and nitrogene ; by adding a little water to the residuum, they procured the remaining two fifths, and found in the vessel in which the operation was carried on, nothing but potash. — Again, it is stated, that by treating a new quantity of metal with the ammonia disengaged from the fusible substance, they again obtained hydrogene, and an absorption of the ammonia ; and by carrying on the operation, they affirm, that they can procure from a given quantity of ammonia, more than its volume of hydrogene.

Whence, they ask, can the hydrogene proceed ?—shall it be

* Mem. d'Arcueil, Tom. II. page 309.

admitted that it is from the ammonia? but this, say they, is impossible; for all the ammonia is reproduced. It must then come from the water which may be supposed to be in the ammonia, or from the metal itself. But the experiments of M. BERTHOLLET, jun. prove that ammonia does not contain any sensible quantity of water. Therefore, say they, the hydrogene gas must be produced from the metal; and as, when this gas is separated, the metal is transformed into potash, the metal appears to be nothing more than a combination of hydrogene, and that alkali.

It is obvious, that even supposing the statement of these gentlemen correct, their conclusions may easily be controverted. They affirm that all the ammonia is reproduced; but they do not obtain it without the addition of *water*. And of the oxygene which this would give to the potassium, and of the hydrogene which it might furnish, to reproduce the ammonia, they take no notice.

I have shewn, by numerous experiments, many of which have been repeated before members of this Society, that the results obtained, by applying heat to the fusible substance, are very different from those stated by the ingenious French chemists, when the operations are conducted in a refined and accurate manner.

In proportion as more precautions are taken to prevent moisture from being communicated to it, so in proportion, is less ammonia regenerated; and I have seldom obtained as much as $\frac{1}{10}$ of the quantity absorbed. And I have never procured hydrogene and nitrogene, in the proportions in which they exist in ammonia; but there has been always an excess of nitrogene.

The processes which I have detailed in the last Bakerian lecture, and in the appendix to it, shew this; and they like-wise shew that a considerable quantity of potassium is always revived.

I have lately performed the experiments, in a manner which I proposed, page 458 of the last volume of the Transactions, and the results have been very satisfactory; as far as they relate to the question of the nature of potassium.

I employed a tube of platina bored from a single piece, which having a stop-cock and adaptor of brass, connected with the mercurial apparatus, could be used as a retort; the potassium was employed in quantities of from 3 to 4 grains, and the absorption of the ammonia conducted as usual, in a retort of glass free from metallic oxides; and in a tray of platina.

In some of the processes, in which the heat was rapidly applied, some of the gray matter, which I have formerly described as a pyrophorus, passed over in distillation, and in these cases, there was a considerable deficiency of hydrogene, as well as nitrogene, in the results of the experiment; but when the heat was very slowly raised, the loss was much less considerable, and in several cases, I obtained more than four fifths of the potassium which had been employed; and very nearly the whole of the nitrogene, existing in the am-monia that had been acted upon.

I shall give an account of one process, conducted with scru-pulous attention. The barometer was at 30.2^{in}, thermometer at $54°$ Fahrenheit. Three grains and a half of potassium were heated in 12 cubical inches of ammonia, 7.5 were ab-

sorbed, and 3.2 of hydrogene evolved. The fusible substance was not exposed to the atmosphere, but was covered with dry mercury, and immediately introduced into the tube; which, with its adaptors was exhausted, and filled with hydrogene. They contained together $\frac{8}{10}$ of a cubical inch. The heat was very slowly applied by means of a fire of charcoal, till the tube was ignited to whiteness. Nine cubical inches of gas were given off, and $\frac{1}{2}$ of a cubical inch remained in the retort and adaptors. Of the 9 cubical inches, $\frac{1}{5}$ of a cubical inch was ammonia, 10 measures of the permanent gas, mixed with 7.5 of oxygene, and acted upon by the electrical spark, left a residuum of 7.5. The quantity of potassium formed, was such as to generate by its action upon water, three cubical inches, and $\frac{3}{10}$ of hydrogene gas.

Now if this experiment be calculated upon, it will be found, that $7.5 - .2 =$ to 7.3 of ammonia, by its electrical decomposition, would afford about 13.1 of permanent gas, containing 3.4 of nitrogene, and 9.7 of hydrogene. But the 3.2 cubical inches of hydrogene, evolved in the first part of the process, added to the 5.8 evolved in the second part of the process $=$ 9; and the nitrogene in the 8.8 cubical inches of gas, (or the 9—.2 of ammonia,) will be about 3, and if we estimate .34 of hydrogene, and .16 of nitrogene, in the .5, remaining in the retort; there will be very little difference in the results of the analysis of ammonia by electricity, and by the action of potassium ; and calculating upon the $\frac{8}{10}$ of hydrogene pre-existing in the tube and adaptors, the loss of hydrogene will be found proportionally rather greater than that of nitrogene.

In another experiment in which 3 grains of potassium were employed in the same manner, 6.78 cubical inches of ammonia

were found to be absorbed, and 2.48 of hydrogene only generated. The distillation was performed, the adaptors and tube being full of common air : 8 cubical inches of gas were produced ; and there must have remained in the tubes and adaptors, the same quantity of residual air, as in the process last described.

The 8 cubical inches of gas contained scarcely $\frac{2}{5}$ of a cubical inch of ammonia ; and the unabsorbable part detonated with oxygene, in the proportion of 11 to 6, gave a residuum of 7.5. — The barometer was at 30.2^{in}, thermometer at 52° Fahrenheit. Dr. Pearson, Mr. Allen, and Mr. Pepys were present during the whole of these operations, and kindly assisted in the progress of them.

Now 6.78 — .4 of ammonia $= 6.38$, and this quantity of gas decomposed by electricity, would afford 11.4 of permanent gas, consisting of 2.9 nitrogene, and 8.5 hydrogene; but there are produced in this experiment, of hydrogene, 2.48 in the first operation, and 4.28 in the second, and considering the nitrogene in the permanent gas as 3.32, .8 must be subtracted for the common air; which would give 2. 52 for the nitrogene generated ; and to these must be added, the quantity of hydrogene and nitrogene in the tubes and adaptors.

The quantity of potassium regenerated, was sufficient to produce 2.9 cubical inches of hydrogene.

In all experiments of this kind, a considerable quantity of black matter separated, during the time the potassium in the tube was made to act upon water.

This substance was examined. It was in the state of a fine powder. It had the lustre of plumbago, it was a conductor of electricity. When it was heated, it took fire at a temperature

below ignition; and after combustion, nothing remained but minutely divided platina.

I exposed some of it to heat in a retort, containing oxygene gas; there was a diminution of the gas; and a small quantity of moisture condensed on the upper part of the retort, which proved to be mere water.

I made two or three experiments, with a view to ascertain the quantity of this substance formed, and to determine more fully its nature. I found that in the process in which from 3 to 4 grains of potassium, were made to act upon ammonia in a vessel of platina, and afterwards distilled in contact with platina, there were always from 4 to 6 grains of this powder formed; but I have advanced no further in determining its nature, than in ascertaining, that it is platina combined with a minute quantity of matter, which affords water by combustion in oxygene.

In the processes on the action of potassium and ammonia, in which iron tubes were used, as appears from the experiments detailed in the last Bakerian lecture, and the appendix, there is always a loss of nitrogene, a conversion of a portion of potassium into potash, and a production of hydrogene. When copper tubes are employed, the hydrogene bears a smaller proportion to the nitrogene; and more potassium is revived.

In these experiments, in which platina has been used, there is little or no loss of potassium or nitrogene; but a loss smaller, or greater of hydrogene.

It will be asked, on what do these circumstances depend? Do the affinity of certain metals for potassium, prevent it from gaining oxygene from ammonia, and do platina and copper

combine with a small quantity of hydrogene, or its basis? Or are there some sources of inaccuracy in those processes, in which nitrogene has appeared to be decomposed? The discussion of these difficult problems will be considered in that part of this lecture, in which the nature of ammonia will be illustrated by some new experiments. The object of the present part of the enquiry is the demonstration of a part of chemical doctrine, no less important and fundamental to a great mass of reasoning, namely, that by the operation of potassium upon ammonia, it is not *a metallic* body that is decompounded, but the volatile alkali, and that the *hydrogene* produced does not arise from the potassium, as is asserted by the French chemists, but from the *ammonia*, as I have always supposed; the potassium in the most refined experiments is *recovered*, but neither the ammonia nor its elements can be reproduced, except by introducing a new body, which contains oxygene and hydrogene.

I have made an experiment upon the action of sodium on ammonia, with the same precautions as in the experiments just detailed, a tray, and the same tube of platina being employed.

3. $\frac{3}{10}$ grains of sodium I found absorbed 9.1 of ammonia, and produced about 4.5 of hydrogene, and the fusible substance, which was very similar to that from potassium, distilled, did not give off $\frac{1}{20}$ of the ammonia that had disappeared; and this small quantity I am inclined to attribute to the presence of moisture. The permanent gas produced, equalled twelve cubical inches, and by detonation with oxygene, proved to consist of nearly two of hydrogene, to one of nitrogene. Sodium was regenerated, but an accident prevented me from ascertaining the quantity.

Whoever will consider with attention, the mere visible phænomena of the action of sodium on ammonia, cannot, I conceive, fail to be convinced that it is the volatile alkali, and not the metal which is decompounded in this process.

As sodium does not act so violently upon oxygene, as potassium; and as soda does not absorb water from the atmosphere, with nearly so much rapidity as potash, sodium can be introduced into ammonia, much freer from moisture, than potassium. Hence, when it is heated in ammonia, there is no effervescence, or at least one scarcely perceptible. Its tint changes to bright azure, and from bright azure to olive green, it becomes quietly and silently converted into the fusible substance, which forms upon the surface, and then flows off into the tray. It emits no elastic fluid, and gains its new form evidently, by combining with one part of the elementary matter of ammonia, whilst another part is suffered to escape in the form of hydrogene.

It will not be necessary for me to enter into a very minute experimental examination of the opinion of M. CURAUDAU, that the metals of the alkalies, are composed of the *alkalies* merely united to *charcoal;* the investigation upon which he has founded his conclusions, is neither so refined, nor so difficult, as that which has been just examined. This gentleman has been misled by the existence of charcoal, as an accidental constituent in the metals he employed, in a manner much, more obvious, than that in which MM. GAY LUSSAC and THENARD have been misled by the moisture which interfered with their results.

M. CURAUDAU states, that when sodium is oxydated, carbonic acid is formed. This I have never found to be the

case, except when the sodium was covered by a film of naphtha. I burnt two grains of sodium in 8 cubical inches of oxygene: nearly two cubical inches of oxygene were absorbed, and soda in a state of extreme dryness, so that it could not be liquified by a heat below redness, formed. This soda did not give out an atom of carbonic acid, during its solution in muriatic acid. Three grains of sodium were made to act upon water; they decomposed it with the phænomena which I have described in the Bakerian lecture for 1807. Nearly 6 cubical inches of hydrogene were produced. No charcoal separated; no carbonic acid was evolved, or found dissolved in the water. Whether the metals of potash or soda were formed by electricity, or by the action of ignited iron on the alkalies, the results were the same. When charcoal is used in experiments on potassium or sodium, they usually contain a portion of it in combination, and it appears from M. Curau-dau's method of decomposing the alkalies, that his metals must have been carburets not of potash and soda, but of potassium and sodium.

M. Ritter's argument in favour of potassium and sodium, being compounds of hydrogene, is their extreme lightness. This argument I had in some measure anticipated, in my paper on the decomposition of the earths; no one is more easily answered. Sodium absorbs much more oxygene than potassium, and on the hypothesis of hydrogenation, must contain much more hydrogene; yet though soda is said to be lighter than potash, in the proportion of 13 to 17 nearly,* yet sodium is heavier than potassium in the proportion of 9 to 7 at least.

* Hassenfratz, Annal. de Chem. Tom. XXVIII. page 11.

On the theory which I have adopted, this circumstance is what ought to be expected. Potassium has a much stronger affinity for oxygene than sodium ; and must condense it much more, and the resulting higher specific gravity of the combination is a necessary consequence.

M. Ritter has stated, that of all the metallic substances he tried for producing potassium by negative Voltaic electricity, tellurium was the only one by which he could not procure it. And he states the very curious fact, that when a circuit of electricity is completed in water, by means of two surfaces of tellurium, oxygene is given off at the positive surface, no hydrogene at the negative surface, but a brown powder which he regards as a hydruret of tellurium, is formed and separates from it ; and he conceives that the reason why tellurium prevents the metallization of potash is, that it has a stronger attraction for hydrogene, than that alkali.

These circumstances of the action of tellurium upon water, are so different from those presented by the action of other metals, that they can hardly fail to arrest the attention of chemical enquirers. I have made some experiments on the subject, and on the action of tellurium on potassium, and I find that instead of proving that potassium is a compound of potash and hydrogene, they confirm the idea of its being as yet like other metals undecompounded.

When tellurium is made the positive surface in water, oxygene is given off, when it is made the negative surface, the Voltaic power, being from a battery composed of a number of plates exceeding 300 ; a purple fluid is seen to separate from it, and diffuse itself through the water ; the water gradually becomes opaque and turbid, and at last deposits a brown

powder. The purple fluid is, I find, a solution of a compound
of tellurium and hydrogene in water; which in being diffused,
is acted upon by the oxygene of the common air, dissolved in
the water, and gradually loses a part of its hydrogene, and
becomes a solid hydruret of tellurium. The compound of hy-
drogene and tellurium produced at the negative pole when
uncombined is gaseous at common temperatures, and when
muriatic acid, or sulphuric acid are present in the water, it is not
dissolved, but is given off, and may be collected and examined.

I acted upon potash by means of a surface of tellurium,
negatively electrified, by a part of the large VOLTAIC apparatus
lately constructed on a new plan in the laboratory of the Royal
Institution, an account of which, with figures, will be found
annexed to this paper. 1000 double plates were used. The
potash was in the common state, as to dryness. There was a
most violent action, and a solution of the tellurium, with much
heat, and a metallic mass, not unlike nickel in colour, was
formed; which when touched by water, did not inflame nor
effervesce, but rendered the water of a beautiful purple
colour, and when thrown into water entirely dissolved, making
a bright purple tincture. It immediately occurred to me,
that the whole of the hydrogene, which in common cases
would have been furnished from the decomposition of the
water, had in this instance combined with he tellurium, and
that the *telluretted* hydrogene, (if the name may be used,) had
formed with the oxidated potassium, i. e. the potash, a peculiar
compound, soluble in water; and this I found to be the case;
for on pouring a little diluted muriatic acid into the mixture,
it effervesced violently, and gave a smell very like that of
sulphuretted hydrogene; metallic tellurium was formed where

it came in contact with the air, and muriate of potash was found dissolved in the mixture.

It seemed evident from this fact, that in the action of tellurium negatively electrified upon potash, potassium was produced as in all other cases, and that it combined with the tellurium, and formed a peculiar alloy ; and this opinion was farther confirmed, by the immediate action of potassium upon tellurium. When these metals were gently heated in a retort of green glass, filled with hydrogene gas, they combined with great energy, producing most vivid heat and light, and they composed an alloy of a dark copper hue, brittle, infusible at a heat below redness, and possessing a crystalline fracture. When the tellurium was in excess in this mixture, or even nearly equal to the potassium in quantity, no hydrogene was evolved by the action of the alloy upon water ; but the compound of telluretted hydrogene and potash was formed, which remained dissolved in the fluid, and which was easily decomposed by an acid.

The very intense affinity of potassium and tellurium for each other, induced me to conceive that the *decomposition of potash*, might be easily effected, by acting on the oxide of tellurium and potash at the same time, by heated charcoal ; and I soon proved that this was the case. About 100 grains of oxide of tellurium, and 20 of potash, were mixed with 12 grains of well burnt charcoal in powder, and heated in a green glass retort ; before the retort became red there was a violent action, much carbonic acid was given off, a vivid light appeared in the retort, and there was found in it the alloy of tellurium and potassium.

In attempting to reduce some oxide of tellurium by charcoal,

which Mr. Hatchett had the kindness to give me for the pur-
poses of these experiments, and which must have been precipi-
tated by potash, or from a solution in potash, I found that a
sufficient quantity of alkali adhered to it, even after it had been
well washed, to produce an alloy of potassium and tellurium ;
but in this alloy the potassium was in very small quantity. It
was of a steel gray colour, very brittle, and much more fusible
than tellurium.

I shall not arrest the progress of discussion, by entering at
present into a minute detail of the properties of the *aeriform*
compound of tellurium and hydrogene ; I shall mention
merely some of its most remarkable qualities, and agencies,
which, as will be shewn towards the close of this paper, tend
to elucidate many points immediately connected with the sub-
ject in question. The compound of tellurium and hydrogene,
is more analogous to sulphuretted hydrogene, than to any
other body. The smell of the two substances is almost
precisely the same.* Its aqueous solution is of a claret colour ;

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* In some experiments, made on the action of tellurium and potassium, in the la-
boratory of my friend John George Children, Esq. of Tunbridge, in which Mr.
Children, Mr. Pepys, and Mr. Warburton co-operated, the analogy between
the two substances struck us so forcibly, as for some time to induce us to conceive
that *tellurium* might contain *sulphur,* not manifested in any other way but by the
action of Voltaic electricity, or by potassium ; and some researches made upon the
habitudes of different metallic sulphurets, at the Voltaic negative surface, rather
confirmed the suspicion, for most of the sulphurets that we tried, which were conductors
of electricity, absorbed hydrogene in the voltaic circuit. The great improbability, how-
ever, of the circumstance that sulphuric acid, or sulphur in any state of oxygenation
could exist in a metallic solution, which was not manifested by the action of barytes,
induced me to resist the inference ; and further researches, made in the laboratory of
the Royal Institution, proved that the substance in question was a new and singular
combination.

but it soon becomes brown, and deposits tellurium, by exposure to air. When disengaged from an alkaline solution by muriatic acid, it reddens moistened litmus; but after being washed in a small quantity of water, it loses this property; but in this case likewise it is partially decomposed by the air in the water; so that it is not easy to say, whether the power is inherent in it, or depends upon the diffusion of a small quantity of muriatic acid through it. In other respects, it resembles a weak acid, combining with water, and with the alkalies. It precipitates most metallic solutions. It is instantly decomposed by oxymuriatic acid, depositing a film, at first metallic; but which is soon converted into muriate of tellurium.*

As arsenic has an affinity for hydrogene, it occurred to me as probable, that it would present some phænomena analogous to those offered by tellurium, in its action upon potassium, and in its operation upon water, when electrified.

Arsenic made the negative surface, in water, by means of a part of the new battery, containing 600 double plates, became dark coloured, and threw down a brown powder; but it likewise gave off a considerable quantity of inflammable gas.

Arsenic negatively electrified in a solution of potash, likewise afforded elastic matter; but in this case the whole solution took a deep tint of brown, and was pellucid; but it became turbid, and slowly deposited a brown powder, by the action of an acid. When arsenic was made the negative surface, in

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* From the results of one experiment which I tried, it seems that tellurium, merely by being heated strongly in dry hydrogene, enters into combination with it. An accident prevented me from ascertaining whether the compound so formed, is exactly the same as that described in the text.

contact with solid potash, an alloy of arsenic and potassium was formed of a dark gray colour, and perfectly metallic; it gave off arseniuretted hydrogene by the action of water with inflammation, and deposited a brown powder.

When potassium and arsenic * were heated together in hydrogene gas, they combined with such violence as to produce the phænomena of inflammation, and an alloy was produced of the same kind as that formed by means of the VOLTAIC battery.

As tellurium and arsenic both combine with hydrogene, it appeared to me probable, that by the action of alloys of potassium, with tellurium and arsenic, upon ammonia, some new phænomena would be obtained, and probably, still further proofs of the decomposition of the volatile alkali, in this process afforded; and this I found was actually the case.

When the easily fusible alloy of tellurium with potassium, in small quantity, was heated in ammonia, the surface lost its metallic splendour, and a dark brown matter was formed, which gave ammonia by exposure to air; and the elastic fluid, which was generated in this operation, consisted of four-sixths nitrogene, instead of being pure hydrogene, as in the case of the action of potassium alone.

The alloy of arsenic and potassium, by its action upon

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* In reasoning upon the curious experiment of CADET, of the production of a volatile pyrophorus by the distillation of acetite of potash, and white oxide of arsenic, Fourcroy Connais. Chem. Tom. viii. p. 197, I conceived it probable, that this pyrophorus was a volatile alloy of potassium and arsenic. But from a repetition of the process I find, that though potash is decompounded in this operation, yet that the volatile substance is not an alloy of potassium, but contains charcoal and arsenic, probably with hydrogene. The gasses not absorbable by water given off in this operation, are peculiar. Their smell is intensely fœtid. They are inflammable, and seem to contain charcoal, arsenic, and hydrogene: whether they are mixtures of various gasses, or a single compound, I am not at present able to decide.

ammonia, likewise produced a gas which was principally ni-trogene, so that if it be said that the metal, and not the volatile alkali is decomposed in processes of this kind, it must be considered in some cases as a compound of nitrogene, and in others a compound of hydrogene, which are contradictory assumptions.

None of the chemists who have speculated upon the *ima-ginary hydrogenation* of potash, as far as my knowledge ex-tends, have brought forward any arguments of analysis, or synthesis. Their reasonings have been founded, either upon distant analogies, or upon experiments in which agents, which they did not suspect were concerned. No person, I be-lieve, has attempted to shew that when potassium or sodium is burnt in oxygene gas, water is formed, or that water is generated, when potassium decomposes any of the acids,* and no one has been able to form potassium, by combining hy-drogene with potash. I stated in the Bakerian lecture for 1807, that when potassium and sodium were burnt in oxygene

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* When in October 1807, I obtained a dark coloured combustible substance from boracic acid, at the negative pole in the VOLTAIC circuit, I concluded that the acid was probably decomposed, according to the common law of electrical decomposition. In March 1808, I made further experiments on this substance, and ascertained that it produced acid matter by combustion ; and I announced the decomposition in a pub-lic lecture delivered in the Royal Institution March 12. Soon after I heated a small quantity of potassium, in contact with dry boracic acid, no water was given off in the operation, and I obtained the same substance as I had procured by electricity. MM. GAY LUSSAC and THENARD have likewise operated upon boracic acid, by potas-sium, and they conclude that they have decompounded it; but this does not follow from their theory, unless they prove that water is given off in the operation, or combined with the borate of potash ; the legitimate conclusion to be drawn from the processes, on their hypothesis, was, that they had made a hydruret of boracic acid.

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gas, *the pure* alkalies were formed in a state of extreme dryness ; and that 100 parts of potassium, absorb about 18 parts of oxygene, and 100 parts of soda about 34 parts. Though in the experiments from which these deductions were made, very small quantities only of the materials were employed, yet still from frequent repetitions of the process, I hoped that they would approach to accuracy ; and I am happy to find that this is the case ; for the results differ very little in some experiments which I have made, upon considerable portions of potassium and sodium, procured by chemical decomposition.

When potassium is burnt in trays of platina, in oxygene gas that has been dried by ignited potash, the absorption of oxygene is about $\frac{11}{20}$ of a cubical inch for every grain of the metal consumed ; and when sodium is burnt in a similar manner, about a cubical inch is taken up for every grain.* The alkalies so formed, are only imperfectly fusible at a red heat ; and do not, like the easily fusible alkalies, give indications of the presence of moisture.

M. D'Arcet has shewn by some very well conducted enquiries, that potash and soda,† in their common state, contain a considerable proportion of water ; and M. Berthollet concludes, that 100 parts of potash, that have been kept for some time in fusion, contain 13.89 parts of water, which is lost when the alkali enters into combination with muriatic acid ; and the same sagacious observer, from some very minute experiments, infers, that muriate of potash, which has been

* The quantities of gas given out by the operation of water, are in a similar ratio. See page 43 of the last Baker an lecture, and page 26 of this paper.

† Annales de Chimie. Nov. 1808, page 175.

ignited, contains in the 100 parts 66.66 potash, and 33.34 muriatic acid, a determination which differs very little from that of BUCHOLZ.

To determine the relation of the dryness of the potash, formed from potassium, to that which has been considered as freed from the whole or the greatest part of its water, in muriate of potash, I made several experiments. I first attempted to convert a certain quantity of potassium into potash, upon the surface of liquid muriatic acid; but in this case the heat was so intense, and hydrogene holding potassium in solution, was disengaged with so much rapidity, that there was a considerable loss of alkali; yet even under these circumstances, I obtained from 10 parts of potassium, 17.5 of dry muriate of potash. The most successful and the only mode which I employed, that can be entirely depended upon, was that of converting potassium into muriate of potash, in muriatic acid gas. I shall give the results of two experiments made in this manner : 5 grains of potassium inserted in a tray of platina, were made to act upon 19 cubical inches of muriatic acid gas, that had been exposed to muriate of lime ; by the application of a gentle heat, the potassium took fire, and burnt with a beautiful red light ;* and the whole mass appeared in igneous fusion ; a little muriate of potash in the state of a white powder, sublimed and collected in the top of the vessel in which the experiment was made. Nearly 14 cubical inches of muriatic acid gas were absorbed, and about 5 of hydrogene were produced. The increase of weight of the tray was about 4.5 grains; and it did not lose any weight by being ignited.

* As a retort exhausted of common air was used, the small quantity of residual common air may have been connected with this vividness of combustion.

The second experiment was conducted with still more attention to minuteness. 8 grains of potassium were employed; above 22 cubical inches of muriatic acid gas were consumed; the potassium burnt with the same brilliant phænomena as in the last experiment, and the increase of weight of the tray was $6\frac{1}{2}$ grains. The muriate of potash was kept for some minutes in fusion in the tray, till a white fume began to rise from it, but it did not lose the $\frac{1}{20}$ of a grain in weight. After the muriate of potash had been washed out of the tray, and it had been cleaned and dried, it was found to have lost about a third of a grain, which was platina in a metallic state, and that had alloyed with the potassium where it was in contact with the tray, during the combustion. There was no appearance of any water being separated in the process. A little muriate of potash sublimed; this was washed out of the retort, and obtained by evaporation: it did not equal $\frac{1}{8}$ of a grain.

Now if the data for calculation be taken from this last experiment, 8 grains of potassium will combine with 1.4 grains of oxygene, to form 9.4 grains of potash, and 6.6 — 1.4 = 5.2, the quantity of muriatic acid combined with the potash, which would give in the 100 parts in muriate of potash, 35.6 of acid, and 64.4 of potash; but 35.6 of muriatic acid according to M. BERTHOLLET's estimation, would demand 71.1 of alkali, in the state of dryness in which it exists in muriate of potash, and 71.1 — 64.4 = 6.7 — so that the potash taken as a standard by M. BERTHOLLET, contains at least 9 per cent. more water, than that existing in the potash formed by the combustion of potassium in muriatic acid gas, which consequently may with much more propriety be regarded as the dry alkali.*

* Consequently M. BERTHOLLET's fused potash must contain nearly 23 per cent.

After these illustrations, I trust the former opinions which I ventured to bring forward, concerning the metals of the fixed alkalies, will be considered as accurate, and that potassium and sodium, can with no more propriety be considered as *compounds*, than any of the common *metallic substances;* and that potash and soda, as formed by the combustion of the metals, are pure metallic oxides, in which no water is known to exist.

These conclusions must be considered as entirely independent of hypothetical opinions, concerning the existence of hydrogene in combustible bodies, as a common principle of inflammability, and of intimately *combined water*, as an essential constituent of acids, alkalies, and oxides; this part of the enquiry I shall reserve for the conclusion of the lecture, and I shall first consider the nature of the metal of ammonia, and the metals of the earths.

III. *Experiments on Nitrogene, Ammonia, and the Amalgam from Ammonia.*

ONE of the queries that I advanced, in attempting to reason upon the singular phænomena produced by the action of potassium upon ammonia was, that nitrogene might possibly consist of oxygene and hydrogene, or that it might be composed from water.

I shall have to detail in this section, a great number of laborious experiments, aud minute and tedious processes, made with the hopes of solving this problem. My results have

of water. From my own observations I am inclined to believe, that potash kept for some time in a red heat, contains 16 or 17 per cent. of water, taking the potash formed by the combustion of potassium, as the dry standard.

been for the most part negative ; but I shall venture to state
them fully, because I hope they will tend to elucidate some
points of discussion, and may prevent other chemists from
pursuing the same paths of enquiry, and which at first view
do not appear unpromising.

The formation of nitrogene has been often asserted to take
place in many processes, in which none of its known combi-
nations were concerned. It is not necessary to enter into the
discussion of the ideas entertained by the German chemists,
on the origin of nitrogene, produced during the passage of
water through red hot tubes, or the speculations of GIRTANNER,
founded on these and other erroneous data; the early dis-
covery of PRIESTLEY on the passage of gasses through red
hot tubes of earthen ware, the accurate researches of BER-
THOLLET, and the experiments of BOUILLON LA-GRANGE, have
afforded a complete solution of this problem.

One of the most striking cases, in which nitrogene has been
supposed to appear without the presence of any other matter
but water, which can be conceived to supply its elements,
is in the decomposition and recomposition of water by elec-
tricity.* To ascertain if nitrogene could be generated in this
manner, I had an apparatus made, by which a quantity of
water could be acted upon by VOLTAIC electricity, so as to pro-
duce oxygene and hydrogene with great rapidity, and in
which these gasses could be detonated, without the exposure
of the water to the atmosphere ; so that this fluid was in con-
tact with platina, mercury, and glass only ; and the wires for
completing the VOLTAIC, and common electrical circuit, were

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* See Dr. PEARSON's elaborate experiments, on the decomposition of water by
electrical explosions. NICHOLSON's Journal, 4to. vol. I. page 301.

hermetically inserted into the tube. 500 double plates of the VOLTAIC combination were used, in such activity that about the eighth of a cubical inch of the mixed gasses, upon an average, was produced from 20 to 30 times in every day. The water used in this experiment was about a half a cubic inch; it had been carefully purged of air by the air-pump and by boiling, and had been introduced into the tube, and secured from the influence of the atmosphere whilst warm. After the first detonation of the oxygene and hydrogene, which together equalled about the eighth of a cubical inch, there was a residuum of about $\frac{1}{40}$ of the volume of the gasses; after every detonation this residuum was found to increase, and when about 50 detonations had been made, it equalled rather more than $\frac{1}{4}$ of the volume of the water i. e. $\frac{1}{8}$ of a cubical inch. It was examined by the test of nitrous gas; it contained no oxygene; 6 measures mixed with 3 measures of oxygene diminished to 5; so that it consisted of 2.6 of hydrogene, and 3.4 of a gas, having the characters of nitrogene.

This experiment seemed in favour of the idea of the production of nitrogene from pure water in these electrical processes; but though the platina wires were hermetically sealed into the tube, it occurred to me as possible that at the moment of the explosion by the electrical discharge, the sudden expansions and contractions might occasion some momentary communication with the external air through the aperture; and I resolved to make the experiments in a method by which the atmosphere was entirely excluded. This was easily done by plunging the whole of the apparatus, except the upper parts of the communicating wires under oil, and carrying on the process as before. In this experiment the residuum did not

seem to increase quite so fast as in the former one. It was carried on for nearly two months. After 340 explosions, the permanent gas equalled $\frac{24}{100}$ of a cubical inch. It was carefully examined : six measures of it detonated with 3 measures of oxygene, diminished to rather less than 1 measure. A result which seems to shew, that nitrogene is not formed during the electrical decomposition and recomposition of water, and that the residual gas is hydrogene. That the hydrogene is in excess, may be easily referred to a slight oxidation of the platina.

The refined experiments of Mr. Cavendish on the deflagration of mixtures of oxygene, hydrogene, and nitrogene, lead directly to the conclusion, that the nitrous acid sometimes generated in experiments on the production of water, owes its origin to nitrogene, mixed with the oxygene and hydrogene, and is never produced from those two gasses alone. In the Bakerian lecture for 1806, I have stated several facts, which seem to shew that the nitrous acid, which appears in many processes of the Voltaic electrization of water, cannot be formed, unless *nitrogene* be present.

Though in these experiments I endeavoured to guard with great care against all causes of mistake, and though I do not well see how I could fall into an error, yet I find that the assertion, that both acids and alkalies may be produced from pure water, has again been repeated.[*] The energy with which the large Voltaic apparatus, recently constructed in the Royal Institution, acts upon water, enabled me to put this question to a more decided test, than was before in my power. I had formerly found in an experiment, in which pure water

[*] Nicholson's Journal, August, 1809, p. 258.

was electrified in two gold cones in hydrogene gas, that no nitrous acid nor alkali was formed. It might be said, that in this case the presence of hydrogene dissolved in the water, would prevent nitrous acid from appearing; I therefore made two series of experiments, one in a jar filled with oxygene gas, and the other in an apparatus, in which glass, water, mercury, and wires of platina only, were present.

In the first series 1000 double plates were used, the two cones were of platina, and contained about $\frac{1}{12}$ of a cubical inch each, and filaments of asbestus were employed, to connect them together. In these trials, when the batteries were in full action, the heat was so great, and the gasses were disengaged with so much rapidity, that more than half the water was lost in the course of a few minutes. By using a weaker charge, the process was carried on for some hours, and in some cases, for from two to three days. In no instance, in which slowly distilled water was employed, and in which the receiver was filled with pure oxygene, from oxymuriate of potash, was any acid or alkali exhibited in the cones; even when nitrogene was present, the indications of the production of acid and alkaline matter, were very feeble; though if the asbestus was touched with unwashed hands, or the smallest particle of neutro saline matter introduced, there was an immediate separation of acid and alkali, at the points of contact of the asbestus with the platina, which could be made evident by the usual tests.

In the second series of experiments, the oxygene and hydrogene produced from the water, were collected under mercury, and the two portions of water communicated directly with each other. In several trials made in this way, with a

combination of 500 plates, and continued for some days, it was always found that fixed alkali, separated in the glass negatively electrified; and a minute quantity of acid, which could barely be made evident by litmus, in the glass positively electrified. This acid rendered cloudy nitrate of silver. Whether its presence was owing to impurities which might rise in distillation with the mercury, or to muriatic acid existing in the glass, I cannot say; but as common salt perfectly dry, is not decomposed by silex, it seems very likely that muriatic acid in its arid state, may exist in combination in glass.

I tried several experiments on the ignition and fusion of platina by Voltaic electricity, in mixtures of the vapour of water and oxygene gas. I thought it possible if water could be combined with *more oxygene,* that this heat, the most intense we are acquainted with, might produce the effect. When the oxygene was mixed with nitrogene, nitrous acid was formed; but when it consisted of the last portions from oxymuriate of potash, there was not the slightest indication of such a result.

Water in vapour was passed through oxide of manganese, made red hot in a glazed porcelain tube, the bore of which was nearly an inch in diameter; in this case a solution of nitrous acid, sufficiently strong to be disagreeably sour to the taste, and which readily dissolved copper, was formed.

This experiment was repeated several times, and when the diameter of the tube was large, with precisely the same results. When red oxide of lead was used instead of oxide of manganese, no acid was however generated; but upon this substance a single trial only was made, and that in a small

tube, so that no conclusion can with propriety be drawn from this failure.

I stated in the last Bakerian lecture, that in attempting to produce ammonia from a mixture of charcoal and pearlash, that had been ignited by the action of water, in the manner stated by Dr. WOODHOUSE, I failed in the trial in which the mixture was cooled in contact with hydrogene. I have since made a number of similar experiments. In general when the mixture had not been exposed to air, there was little or no indication of the production of the volatile alkali; but the result was not so constant as to be entirely satisfactory; and the same circumstances could not be uniformly obtained in this simple form of the experiment. I had an apparatus made, in which the phænomena of the process could be more rigourously examined. Pure potash and charcoal, in the proportion of one to four in weight, were ignited in the middle of a tube of iron, furnished with a system of stopcocks, and connected with a pneumatic apparatus, in such a manner, that the mixture could be cooled in contact with the gas, produced during the operation; and that water exhausted of air, could be made to act upon the cooled mixture, and afterwards, distilled from it; figures of this apparatus, and an account of the manner in which it was used, are annexed to this paper. In this place I shall state merely the general results of the operations, which were carrried on for nearly two months, a variety of precautions being used to prevent the interference of nitrogene from the atmosphere.

In all cases in which the water was brought in contact with the mixture of charcoal and potash, when it was perfectly cool, and afterwards distilled from it by a low heat, it was

found to hold in solution, small quantities of ammonia ; when the operation was repeated upon the same mixture, ignited a second time, the proportion diminished ; in a third operation it was sensible, but in the fourth barely perceptible. The same mixture, however, by the addition of a new quantity of potash, again gained the power of producing ammonia in two or three successive operations ; and when any mixture had ceased to give ammonia, the power was not restored by cooling it in contact with air.

Ammonia was produced in a case in which more than 200 cubical inches of gas had passed over from the action of water upon a mixture, and when the last portions only were preserved in contact with it during the cooling. In a comparative trial it was however found, that considerably more ammonia was produced, when a mixture was cooled in contact with the atmosphere, than when it was cooled in contact with the gas developed in the operation.

I shall not attempt to draw any conclusions from these processes. It would appear from some experiments of M. BERTHOLLET, that nitrogene adheres very strongly to charcoal.* The circumstances that the ammonia ceases to be produced after a certain number of operations, and that the quantity is much greater when free nitrogene is present, are perhaps against the idea that nitrogene is composed in the process. But till the weights of the substances concerned and produced in these operations are compared, no correct decision on the question can be made.

The experiments of Dr. PRIESTLEY upon the production of nitrogene, during the freezing of water, induced that philo-

* Mem. d'Arcueil, Tom. II. page 485.

sopher to conceive, either that water was capable of being converted into nitrogene, or that it contained much more nitrogene than is usually suspected.

I have made some repetitions of his processes. A quantity of water, (about a cubical inch and a quarter,) that had been produced from snow, boiled and inverted over mercury whilst hot, was converted into ice, and thawed in 16 successive operations; gas was produced, but after the first three or four times of freezing, there was no notable increase of the volume. At the end of the experiment, about $\frac{1}{50}$ of a cubical inch was obtained, which proved to be common air.

About four cubical inches of water from melted snow were converted into ice and thawed, four successive times in a conical vessel of wrought iron. At the end of the fourth process, the volume of gas equalled about $\frac{1}{20}$ of the volume of the water. It proved to contain about $\frac{1}{10}$ oxygene, $\frac{3}{10}$ hydrogene, and $\frac{6}{10}$ nitrogene.

Mr. Kirwan observed the fact that when nitrous gas, and sulphuretted hydrogene, are kept in contact for some time, there is a great diminution of volume, and that the nitrous gas becomes converted into nitrous oxide, and that sulphur is deposited which has an ammoniacal smell. I repeated this experiment several times in 1800 with similar results, and I found, that the diminution of the volume of the gasses when they were mixed in equal proportions, was to rather less than $\frac{1}{4}$, which seemed to be nitrous oxide.

In reasoning upon this phænomenon, I saw grounds for a minute investigation of it. Sulphuretted hydrogene, as appears from experiments which I have stated on a former occasion, and from some that I shall detail towards the conclusion of

this lecture, contains a volume of hydrogene, equal to its own. But one of hydrogene demands half its volume of oxygene to convert it into water, and nitrous gas consists of about half a part in volume of oxygene; so that supposing the whole of the hydrogene employed in absorbing oxygene from nitrous gas, nitrogene alone ought to be formed, and not nitrous oxide. Or, if the whole of the gas is nitrous oxide, this should contain all the nitrogene of the nitrous gas, leaving none to be supplied to the ammonia. I mixed together five cubical inches of nitrous gas, and five of sulphuretted hydrogene over mercury, the barometer being at $29.5^{in.}$, thermometer at $51°$ FAHRENHEIT; twelve hours had elapsed before any change was perceived; there was then a whitish precipitate formed, and a deep yellow liquid began to appear in drops, on the inside of the jar, and the volume of the gasses quickly diminished; after two days the diminution ceased, and the volume became stationary; the barometer was at $30.45^{in.}$, and thermometer $52°$ FAHRENHEIT; when it equalled 2.3. The gas proved to be about $\frac{3}{4}$ nitrous oxide, and the remaining fourth was inflammable. An experiment was made expressly to determine the nature of the deep yellow liquid in the jar. It proved to be of the same kind as BOYLE's fuming liquor, the hydrosulphuret of ammonia, but with sulphur in great excess.

In this experiment there was evidently no formation of nitrogene, and these complicated changes, ended in the production of two new compounds; nitrogene, hydrogene; oxygene and sulphur combining to form one; and a part of the nitrogene and oxygene, becoming more condensed, to form another.

Having stated the results of the investigation on the pro-
duction of nitrous acid and of ammonia, in various processes of
chemistry, I shall notice some attempts that I made to de-
compound nitrogene, by agents which I conceived might act
at the same time on oxygene, and on the basis of nitrogene.
Potassium, as I have before stated, sublimes in nitrogene,
without altering it, or being itself changed : but I thought it
possible, that the case might be different, if this powerful
agent were made to act upon nitrogene, assisted by the in-
tense heat and decomposing energy of Voltaic electricity.

I had an apparatus made, by which the Voltaic circuit
could be completed in nitrogene gas, confined by mercury,
by means of potassium and platina. The potassium, in the
quantity of about two or three grains, was placed in a cup
of platina, and by contact with a wire of platina, it could be
fused and sublimed in the gas. The quantity of nitrogene,
was usually about a cubical inch. The battery employed
was always in full action for these experiments, and con-
sisted of one thousand double plates. The phænomena were
very brilliant; as soon as the contact with the potassium was
made there was always a bright light, so intense as to be
painful to the eye ; the platina became white hot ; the potas-
sium rose in vapour; and by increasing the distance of the
cup from the wire, the electricity passed through the vapour
of the potassium, producing a most brilliant flame, of from
half an inch to an inch and a quarter in length ; and the
vapour seemed to combine with the platina, which was thrown
off in small globules in a state of fusion, producing an ap-
pearance similar to that produced by the combustion of iron
in oxygene gas.

In all trials of this kind, hydrogene was produced; and in some of them there was a loss of nitrogene. This at first seemed to lead to the inference that nitrogene is decompounded in the process; but I found that, in proportion as the potassium was introduced more free from a *crust of potash,* which would furnish water and consequently hydrogene in the experiment, so in proportion was there less of this gas evolved; and in a case in which the greatest precautions were taken, the quantity did not equal $\frac{1}{8}$ of the volume of gas, and there was no sensible quantity of nitrogene lost.

The largest proportion of nitrogene which disappeared in any experiment, was $\frac{1}{11}$ of the quantity used; but in this case the crust of potash was considerable, and a volume of hydrogene, nearly equal to $\frac{1}{4}$ of the nitrogene, was produced. It cannot be said, that the nitrogene is *not* decomposed in this operation; but it seems much more likely that the slight loss is owing to its combination with nascent hydrogene, and its being separated with the potassium in the form of the gray pyrophoric sublimate, which I have found is always produced, when potassium is electrized and converted into vapour in ammonia.

The phosphuret of lime in its common state, is a conductor of electricity; and when it was made the medium of communication between the wires of the great battery, it burnt with a most intense light. It was ignited to whiteness in nitrogene gas; a little phosphuretted hydrogene was given off from it, but the nitrogene was not altered; the apparatus was similar to that used for the potassium.

As almost all compounds known to contain hydrogene, are readily decomposed by oxymuriatic acid gas, a mixture was

of nitrogene and oxymuriatic acid gas, was passed through a porcelain tube heated to whiteness ; the products were received in a pneumatic apparatus over water, there was a small loss of nitrogene ; but the greatest part came over densely clouded, and as nitromuriatic acid was found dissolved in the water, no conclusions concerning the decomposition of nitrogene can be drawn from the process.

The general tenour of these enquiries cannot be considered as strengthening in any considerable degree, the suspicion which I formed of the decomposition of nitrogene, by the distillation of the olive coloured substance from potassium and ammonia, in tubes of iron.

In reasoning closely upon the phænomena in this operation, it appears to me indeed possible to account for the loss of nitrogene, without assuming that it has been converted into new matter. Though the iron tubes which I used, were carefully cleaned ; yet still it was not unlikely that a small quantity of oxide might adhere to the welded parts ; the oxygene of which, in the beginning of the process of distillation, might form water with hydrogene, given off from the fusible substance ; which being condensed in the upper part of the tube ; would be again brought into action towards the close of the operation, occasioning the formation, and possibly the absorption of some ammonia, and consequently a loss of nitrogene, and the production of an increased proportion of hydrogene. I have made one experiment, with the hopes of deciding this question, in an iron tube used immediately after the whole internal surface had been cleaned by the borer ; six grains of potassium were used in a tray of iron, nearly thirteen cubical inches of ammonia were absorbed, and about six of hydro-

gene produced. Thirteen cubical inches of gas, were evolved
in the first operation ; which consisted of nearly 1 cubical inch
of ammonia, 4 of nitrogene, and 8 of hydrogene. The por-
tion of gas given off in the second operation, equalled 3.6
cubical inches ; which consisted of 2.5 hydrogene, and 1.1
nitrogene. The potassium produced in the operation, was
sufficient to generate 3.1 cubical inches of hydrogene.

As the iron in these experiments had been heated to intense
whiteness, and must have been very soft ; it was not impossi-
ble, considering the recent experiments of M. HASSENFRATZ,[*]
that the loss of so large a portion of potassium, might depend
upon an intimate union of that body with iron, and its pene-
tration into the substance of the tube. This idea is counte-
nanced by another experiment of the same kind, in which the
heat was raised to whiteness, and the barrel cut into pieces
when cool : on examining the lower part of it, I found in it a very
thin film of potash ; but which I conceive, could scarcely equal
a grain in weight. The pieces of the barrel were introduced
under a jar inverted in water ; at the end of two days nearly
2.3 cubical inches of hydrogene were found to be generated.

In the experiments detailed in page 53 of the last volume
of the Transactions, a loss of nitrogene, and a production of
hydrogene was perceived in a case in which the residuum
from a portion of fusible substance, which had been exposed
to a low red heat, was distilled in a tube of platina ; but in this
case the residuum had been covered by *naptha,* and it is pos-
sible that ammonia might have been regenerated by hydro-
gene from the naptha, and absorbed by that fluid ; and a part
of the hydrogene might likewise proceed from the decompo-

[*] Journal des Mines. Avril 1808. p. 275.

sition of the naphtha; and in several experiments in which I have burnt the entire fusible substance, I have found no loss of nitrogene.

Even the considerable excess of hydrogene, and deficiency of nitrogene, in the processes in which the fusible substance is distilled with a new quantity of potassium, page 451, it is possible to refer to the larger quantity of moisture, which must be absorbed by the fusible substance from the air, during the time occupied in attaching the potassium to the tray, and likewise from the moisture adhering to the crust of potash, which always forms upon the potassium, during its exposure to air.

These objections are the strongest that occur to me, against the mode of explaining the phænomena by supposing nitrogene decomposed in the operation; but they cannot be considered as decisive on this complicated and obscure question, and the opposite view may be easily defended.

Though I have already laid before the Society, a number of experiments upon the decomposition of ammonia, yet I shall not hesitate to detail some further operations which have been conducted according to new views of the subject.

I concluded from the loss of weight taking place in the electrical analysis of ammonia, that water or oxygene was probably separated in this operation; but I was aware that objections might be made to this mode of accounting for the phænomenon.

The experiment of producing an amalgam from ammonia, which regenerated volatile alkali, apparently by oxidation, confirmed the notion of the existence of oxygene in this substance, at the same time it led to the suspicion, that of the two gasses separated by electricity, one, or perhaps both, might

contain metallic matter united to oxygene : and the results of
the distillation of the fusible substance, from potassium and
ammonia, notwithstanding the objections I have made, can
perhaps be explained on such a supposition.

I have made a number of experiments upon the decompo-
sition of considerable quantities of ammonia, both by VOLTAIC
and common electricity ; and I have used an apparatus (of
which a figure is attached to this paper,) in which nothing was
present but the gas, the metals for conveying the electricity,
and glass. The ammonia was introduced by a stopcock which
was cleared of common air, into a globe that was exhausted,
after being filled two or three times with ammonia : the gas
that was used was absolutely pure, the decomposition was
performed without any possibility of change in the volume of
the elastic matter, and the apparatus was such, that the gas
could be exposed to a *freezing mixture*, and the whole weighed
before, and after the experiment.

The object in keeping the volume the same during the de-
composition, was to produce the condensation of any aqueous
vapour, which if formed in small quantity in the operation,
(on the theory of the mechanical diffusion of vapour in
gasses,) might in the common case of decomposition, under
the usual pressure, be in quantity nearly twice as much in the
hydrogene and nitrogene, as in the ammonia.

In all instances it was found, that there was no loss of
weight of the apparatus, nor was there any deposition of
moisture, during or after the electrization ; but the wires were
uniformly tarnished ; and in an experiment in which surfaces
of brass were used, a small quantity of olive coloured matter
formed on the metal, but though in this case nearly 8 cubical

inches of ammonia were decomposed, the weight of the oxi-
dated matter was so minute as to be scarcely sensible. By the
use of a freezing mixture of muriate of lime and ice, which
diminished the temperature to —15°, there was a very feeble
indication given of the addition of hygrometrical moisture.

In these experiments the increase of the gas was uniformly
(within a range of five parts,) from 100 to 185, and the hy-
drogene was to the nitrogene, in the average proportions of
from 73.74 to 27.26 ; the proper corrections being made, and
the precautions before referred to, being taken.*

Assuming the common estimations of the specific gravity
of ammonia, of hydrogene, and nitrogene, the conclusions
which I have advanced in the Bakerian lecture for 1807,
would be supported by these new experiments, but as the

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* Philosophical Transactions 1809, page 459. M. BERTHOLLET, Jun. in the
second volume of the Memoirs of Arcueil, has given a paper on the decomposition of
ammonia, and he enters into an examination of my idea of the oxygene, separated in
the electrical decomposition of ammonia, which he supposes I rate at 20 per cent. and
at the same time he confutes some experiments which he is pleased to attribute to me, of
the combustion of charcoal and iron in ammonia. His arguments and his facts upon
these points, appear to me perfectly conclusive ; but as I never formed such an
opinion, as that 20 of oxygene were separated in the experiment, and never imagined
such results as the combustion of iron and charcoal in ammonia, and never published
any thing which could receive such an interpretation, I shall not enter into any cri-
ticism on this part of his paper. The experiments of this ingenious chemist on the
direct decomposition of ammonia, seem to have been conducted with much care, except
as to the circumstance of his not boiling the quicksilver ; which I conceive has occa-
sioned him to over-rate the increase of volume. At all events a loss of weight is more
to be expected than an increase of weight, in all very refined experiments of this kind.
It is possible that the volume may be exactly doubled, and that the nitrogene may be
to the hydrogene as one to three ; but, neither the numerous experiments of Dr.
HENRY, nor those that I have tried, establish this ; it is one of the hypothetical
inferences that may be made, but it cannot be regarded as an absolute fact.

moisture and oxygene visibly separated, cannot be conceived to be as much as $\frac{1}{11}$ or $\frac{1}{12}$ of the weight of the ammonia; I resolved to investigate more precisely, than I had reason to think had been hitherto done, the specific gravities of the gasses concerned in their dry state; and the very delicate balance belonging to the Royal Institution, placed the means of doing this in my power.

Nitrogene, hydrogene, and ammonia, were dried by a long continued exposure to potash, and were very carefully weighed. Their relative specific gravities proved to be at $30.5^{in.}$ barometer, $51°$ FAHRENHEIT's thermometer.

For nitrogene, the 100 cubical inches   -   -   29.8 grains.

For hydrogene, ditto.   -   -   -   -   2.27

For ammonia   -   -   -   -   -   -   18.4

Now, if these data be calculated upon, it will be found, that in the decomposition of 100 of ammonia, taking even the largest proportions of gasses evolved; there is a loss of $\frac{1}{18}$,* and if the smallest proportion be taken the loss will be nearly $\frac{1}{12}$.

These results and calculations agree with those that I have before given, and with those of Dr. HENRY.

The lately discovered facts in chemistry, concerning the important modifications which bodies may undergo by very slight additions or subtractions of new matter, ought to render us cautious in deciding upon the nature of the process of the electrical decomposition of ammonia.

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* 100 of ammonia at the rate of 185, will give 136.9 of hydrogene, weighing 3.1 grains, and 48.1 of nitrogene weighing 14.33 grains; but 18.4 — 17.4 = 1, and at the rate of 180, 133 of hydrogene weighing 3.01 and 47 of nitrogene, weighing 14 and 18.4 — 17 = 1.4.

It is *possible*, that the minute quantity of oxygene which appears to be separated is not accidental, but a result of the decomposition; and if hydrogene and nitrogene be both oxides of the same basis, the possibility of the production of different proportions of water, in different operations, might account for the variations observed in some cases in their relative proportions; but on the whole, the idea that ammonia is decomposed into hydrogene and nitrogene alone, by electricity, and that the loss of weight, is no more than is to be expected in processes of so delicate a kind, is in my opinion, the most defensible view of the subject.

But if *ammonia* be capable of decomposition into nitrogene and hydrogene, what, it will be asked, is the nature of the matter existing in the amalgam of ammonia? what is the metallic basis of the volatile alkali? These are questions, intimately connected with the whole of the arrangements of chemistry; and they are questions, which, as our instruments of experiment now exist, it will not, I fear, be easy to solve.

I have stated in my former communication on the amalgam from ammonia, that under all the common circumstances of its production, it seems to preserve a quantity of water adhering to it, which may be conceived to be sufficient to oxidate the metal, and to reproduce the ammonia.

I have tried various devices with the hopes of being able to form it from ammonia in a dry state, but without success. Neither the amalgams of potassium, sodium, or barium, produce it in ammoniacal gas; and when they are heated with muriate of ammonia, unless the salt is moist, there is no metallization of the alkali.

I have acted upon ammonia by different metallic amalgams negatively electrified, such as the amalgams of gold and silver, the amalgam of zinc, and the liquid amalgam of bismuth and lead ; but in all these cases the effect was less distinct, than when pure mercury was used.

By exposing the mercury to a cold of —20° Fahrenheit, in a close tube, I have succeeded in obtaining an amalgam in a much more solid state ; yet this decomposed nearly as rapidly as the common amalgam, but it gave off much more gaseous matter ; and in one instance I obtained a quantity which was nearly equal to six times its volume.

The amalgam which I have reason to believe can be made most free from *adhering moisture*, is that of potassium, mercury, and ammonium in a solid state. This, as I have mentioned in my former communication, decomposes very slowly, even in contact with water, and when it has been carefully wiped with bibulous paper, bears a considerable heat without altera-tion. I have lately made several new attempts to distil the ammonium from it, but without success. When it is strongly heated in a green glass tube filled with hydrogene gas, there is always a partial regeneration of ammonia ; but with this ammonia there is from $\frac{5}{10}$ to $\frac{6}{10}$ of hydrogene produced.

As it does not seem possible to obtain an amalgam in an uni-form state, as to adhering moisture, it is not easy to say what would be the exact ratio between the hydrogene and ammonia produced, if no more water was present, than would be decom-posed in oxidating the basis. But in the most refined experi-ments which I have been able to make, this ratio is that of one to two ; and in no instance in which proper precautions are

taken, is it less; but under common circumstances often more. If this result is taken as accurate, then it would follow, that ammonia (supposing it to be an oxide,) must contain about 48 per cent. of oxygene, which, as will be hereafter seen, will agree with the relations of the attractions of this alkali for acids, to those of other salifiable bases.*

If hydrogene be supposed to be a simple body, and nitrogene an oxide, then on the hypothesis above stated, nitrogene would consist of nearly 48 of oxygene, and 34 of basis; but if the opinion be adopted, that hydrogene and nitrogene are both oxides of the same metal, then the quantity of oxygene in nitrogene, must be supposed less.

These views are the most obvious that can be formed, on the antiphlogistic hypothesis of the nature of metallic substances; but if the facts concerning ammonia were to be reasoned upon, independently of the other general phænomena of chemical science, they perhaps might be more easily explained on the notion of nitrogene being a basis, which became alkaline, by combining with one portion of hydrogene, and metallic, by combining with a greater proportion.

* Even in common air, the amalgam evolves hydrogene and ammonia, nearly in these proportions, and in one experiment which I lately tried, there seemed to be no absorption of oxygene from the atmosphere. This circumstance appears to me in favour of the antiphlogistic view of the metallization of the volatile alkali; for if the hydrogene be supposed to be given off from the mercury, and not to arise from the decomposition of water adhering to the amalgam, it might be conceived, that being in the nascent state, it would rapidly absorb oxygene. In my first experiments upon the amalgam, finding that common air to which it had been exposed, gave less diminution with nitrous gas than before, I concluded naturally, that oxygene had been absorbed; but this difference might have arisen partly at least, from the mixture of hydrogene. Whether in any case the amalgam absorbs oxygene gas, is a question for further investigation.

The solution of the question concerning the quantity of matter added to the mercury in the formation of the amalgam, depends upon this discussion ; for if the phlogistic view of the subject be adopted, the amalgam must be supposed to contain nearly twice as much matter, as it is conceived to contain on the hypothesis of deoxygenation. In the last Bakerian lecture, I have rated the proportion at $\frac{1}{12000}$, but this is the least quantity that can be assumed, the mercury being supposed to give off only one and a half its volume of ammonia. If the proportion stated in page 56, be taken as the basis of calculation, which is the maximum that I have obtained, the amalgam would contain about $\frac{1}{1600}$ of new matter, on the antiphlogistic view, and about $\frac{1}{900}$ on the phlogistic view.

I shall have occasion to recur to, and to discuss more fully these ideas, and I shall conclude this section by stating, that though the researches on the decomposition, and composition of nitrogene, which have occupied so large a space in the foregoing pages, have been negative, as to the primary object, yet they may not possibly be devoid of useful applications. It does not seem improbable, that the passage of steam over hot manganese, may be applied to the manufacture of nitrous acid. And there is reason to believe that the ignition of charcoal and potash, and their exposure to water, may be advantageously applied to the production of volatile alkali, in countries where fuel is cheap.

IV. *On the Metals of Earths.*

I HAVE tried a number of experiments with the hopes of gaining the same distinct evidences of the decomposition of

the common earths, as those afforded by the electrochemical processes on the alkalies, and the alkaline earths.

I find that when iron wire ignited to whiteness, by the power of 1000 double plates, is negatively electrified and fused in contact with either silex, alumine or glucine, slightly moistened and placed in hydrogene gas ; the iron becomes brittle and whiter, and affords by solution in acids, an earth of the same kind, as that which has been employed in the experiment.

I have passed potassium in vapour through each of these earths, heated to whiteness in a platina tube : the results were remarkable, and perhaps not unworthy of being fully detailed.

When silex was employed, being in the proportion of about ten grains to four of potassium, no gas was evolved, except the common air of the tube mingled with a little inflammable gas, not more than might be referred to the moisture in the crust of alkali, formed upon the potassium. The potassium[*] was entirely destroyed ; and glass with excess of alkali, was formed in the lower part of the tube ; when this glass was powdered, it exhibited dark specks, having a dull metallic character not unlike that of the protoxide of iron. When the mixture was thrown into water, there was only a very slight effervescence ; but on the addition of muriatic acid to the

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[*] The results of this experiment, are opposed to the idea that potassium is a compound of hydrogene and potash or its basis, for if so, it might be expected that the hydrogene would be disengaged by the attraction of the alkali for silex. In my first experiments on this combination, I operated in an apparatus connected with water, and I found that the potassium produced as much hydrogene as if it had been made to act upon water ; in this case the metal had rapidly decomposed the vapour of the water, which must have been constantly supplied.

water, globules of gas were slowly liberated, and the effect continued for nearly an hour; so that there is great reason to believe, that the silex had been either entirely or partially deoxygenated, and was slowly reproduced by the action of the water, assisted by the slight attraction of the acid for the earth.

When the potassium was in the quantity of six grains, and the silex of four grains, a part of the result inflamed spontaneously as it was taken out of the tube, though the tube was quite cool, and left as the result of its combustion, alkali and silex. The part which did not inflame, was similar in character to the matter which has been just described, it did not act upon water, but effervesced with muriatic acid.

Potassium in acting upon alumine and glucine, produced more hydrogene than could be ascribed to the moisture present in the crust of potash; from which it seems probable that even after ignition, water adheres to these earths.

The results of the action of the potassium, were pyrophoric substances of a dark gray colour, which burnt, throwing off brilliant sparks,* and leaving behind alkali and earth, and which hissed violently when thrown upon water, decomposing it with great violence. I examined the products in two experiments, one on alumine, and one on glucine, in which naphtha was introduced into the platina tube, to prevent combustion; the masses were very friable, and presented small metallic particles, which were as soft as potassium, but so small that they could not be separated, so as to be more minutely examined; they melted in boiling naphtha. Either a part of the

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* The pyrophorus from alum, which I have supposed in the last Bakerian lecture, to be a compound of potassium, sulphur, and charcoal, probably contains this substance likewise.

potassium must have been employed in decomposing the earths in these experiments, or it had entered into combination with them, which is unlikely, and contrary to analogy, and opposed by some experiments which will be immediately related.

Supposing the metals of the earths, to be produced in experiments of this kind, there was great reason to expect that they might be alloyed with the common metals, as well as with potassium. Mercury was the only substance which it was safe to try in the tube of platina. In all cases in which the potassium was in excess, I obtained amalgams by introducing mercury, whilst the tube was hot; but the alkaline metal gave the characters to the amalgam, and though in the case of glucine and alumine, a white matter separated during the action of very weak muriatic acid upon the amalgam, yet I could not be entirely satisfied that there was any of the metals of these earths in triple combination.

Mixtures of the earths with potassium, intensely ignited in contact with iron filings, and covered with iron filings in a clay crucible, gave much more distinct results. Whether silex, alumine, or glucine was used, there was always a fused mass in the centre of the crucible; and this mass had perfectly metallic characters. It was in all cases much whiter and harder than iron. In the instance in which silex was used, it broke under the hammer, and exhibited a crystaline texture. The alloys from alumine and glucine, were imperfectly malleable. Each afforded by solution in acids, evaporation, and treatment with re-agents oxide of iron, alkali, and notable quantities of the earth employed in the experiment.

Though I could not procure decided evidences of the pro-

duction of an amalgam, from the metals of the common earths, yet I suceeeded perfectly by the same method of operating, in making amalgams of the alkaline earths.

By passing potassium through lime and magnesia, and then introducing mercury, I obtained solid amalgams, which consisted of potassium, the metal of the earth employed, and mercury.

The amalgam from magnesia, was easily deprived of its potassium by the action of water. It then appeared as a solid white metallic mass, which by exposure to the air became covered with a dry white powder, and which when acted upon by weak muriatic acid, gave off hydrogene gas in considerable quantities, and produced a solution of magnesia.

By operations performed in this manner, there is good reason to believe, it will be possible to procure quantities of the metals of the alkaline earths, sufficient for determining their nature and agencies, and the quantities of oxygene which they absorb ; and by the solution of the alloys containing the metals of the common earths, it seems probable, that the proportions of metallic matter in these bodies, may likewise be ascertained.

On an hypothesis which I have before brought before the Society, namely, that the power of chemical attraction and electrical action, may be different exhibitions of the same property of matter, and that oxygene and inflammable bodies, are in relations of attraction which correspond to the function of being negative and positive respectively, it would follow that the attractions of acids for salifiable bases, would be inversely, as the quantity of oxygene that they contain, and supposing the power of attraction to be measured by the

quantity of basis which an acid dissolves, it would be easy to infer the quantities of oxygene and metallic matter from the quantities of acid and of basis in a neutral salt. On this idea I had early in 1808, concluded that barytes must contain least oxygene of all the earths, and that the order as to the quantity of inflammable matter, must be strontites, potash, soda, lime, and so on; and that silex must contain the largest quantity of oxygene of all.

If the most accurate analyses be taken, barytes may be conceived to contain about 90.5 * of metal per cent. strontites 86,† lime 73.5,* magnesia, 66.‡

The same proportions would follow from an application of Mr. DALTON's ingenious supposition, § that the proportion of

* Mr. JAMES THOMPSON, NICHOLSON's Journal, 1809, p. 175, and BERTHIER.

† Mr. CLAYFIELD, THOMSON's Chemistry, Vol. II. p. 626, 629.

‡ MURRAY's Chemistry, Vol. III. p. 616.

§ The principle that I have stated of the affinity of an acid for a salifiable basis, being inversely, as the quantity of oxygen contained by the basis, though gained from the comparison of the electrical relations of the earths, with their chemical affinities, in its numerical applications, must be considered merely as a consequence of Mr. DALTON's law of general proportions. Mr. DALTON had indeed, in the spring of 1808, communicated to me a series of proportions for the alkalies and alkaline earths; which in the case of the alkalies, were not very remote from what I had ascertained by direct experiments. M. GAY LUSSAC's principle, that the quantity of acid in metallic salts is directly as the quantity of oxygene, might (as far as it is correct) be inferred from Mr. DALTON's law, though this ingenious chemist states that he was led to it by different considerations. According to Mr. DALTON, there is a proportion of oxygene, the same in all protoxides, and there is a proportion of acid, the same in all neutral salts; and new proportions of oxygene and of acid, are always multiples of these proportions. So that if a protoxide in becoming a deutoxide, takes up more acid, it will be at least double the quantity, and in these cases, the oxygene will be strictly as the acid. Mr. DALTON's law even provides for cases to which M. GAY LUSSAC's will not apply, a deutoxide may combine with a single quantity of acid, or a protoxide with a double quantity. Thus in the insoluble oxysulphat of iron per-

oxygene is the same in all protoxides, and that the quantity of acid is the same in all neutral salts, i. e. that every neutral salt is composed of one particle of metal, one of oxygene, and one of acid.

We are in possession of no accurate experiments on the quantity of acids required to dissolve alumine, glucine, and silex ; but according to Richter's estimation of the composition * of phosphate of alumine, alumine would appear to contain about 56 per cent. of metallic matter.

M. Berzelius,† in a letter which I received from him a few months ago, states, that in making an analysis of cast iron, he found that it contained the metal of silex, and that this metal in being oxidated took up nearly half its weight of oxygene.

If the composition of ammonia be calculated upon, according to the principle above stated, it ought to consist of 53 of metallic matter, and about 47‡ of oxygene, which agrees very nearly with the quantity of hydrogene and ammonia produced from the amalgam.

Though the early chemists considered the earths and the

fectly formed, (as some experiments which I have lately made, seem to shew,) there is probably only a single proportion of acid ; and in the super-tartrite of potash there is only a single quantity of oxygene, and a double quantity of acid. Whether Mr. Dalton's law will apply to *all eases,* is a question which I shall not in this place attempt to discuss.

    * Thomson's Chemistry, Vol. II. p. 581.

    † In the same communication this able chemist informed me, that he had succeeded in decomposing the earths, by igniting them strongly with iron and charcoal.

    ‡ I take the proportions of the volumes from the very curious paper of M. Gay Lussac, on the combinations of gaseous bodies, Mem. d'Arcueil, Tom. II. page 213, and the weights from my own estimation, according to which 100 cubic inches of muriatic acid gas weigh 39 grains, at the mean temperature and pressure, which is very nearly the same as the weight given by MM. Gay Lussac, and Thenard.

metallic oxides as belonging to the same class of bodies, and
the earths as calces which they had not found the means of
combining with phlogiston, and though LAVOISIER insisted
upon this analogy with his usual sagacity, yet still the alkalies,
earths, and oxides, have been generally considered as separate
natural orders. The earths, it has been said, are not precipi-
tated by the triple prussiates, or by the solutions of galls ; *
and the alkalies and alkaline earths, are both distinguished by
their solubility in water ; but if such characters be admitted
as grounds of distinct classification, the common metals must
be arranged under many different divisions ; and the more
the subject is inquired into, the more distinct will the general
relations of all metallic substances appear. The alkalies and
alkaline earths combine with prussic acid, and form compounds
of different degrees of solubility ; and solutions of barytes (as
has been shewn by Dr. HENRY and M. GUYTON,) precipitate
the triple prussiate of potash ; the power of combination is
general, but the compounds formed, are soluble in different
degrees in water. The case is analogous with solutions of galls ;
these, as I have mentioned in a paper published in the Philoso-
phical Transactions for 1805, are precipitated by almost all
neutrosaline solutions ; and they form compounds more or
less soluble in water, more or less coloured, and differently
coloured with all salifiable bases. It is needless to dwell upon
the combinations of the alkalies and earths, with oils, to form
soaps ; and of the earthy soaps, some are equally insoluble
with the metallic soaps. The oxide of tin, and other oxides
abounding in oxygene, approach very near in their general
characters to zircon, silex, and alumine ; and in habits of

* KLAPROTH. Annales de Chimie. Tom. X. p. 277.

amalgamation, and of alloy, how near do the metals of the alkalies approach to the lightest class of oxidable metals?

It will be unnecessary, I trust, to pursue these analogies any further, and I shall conclude this section by a few remarks on the alloys of the metals of the common earths.

It is probable that these alloys may be formed in many metallurgical operations, and that small quantities of them may influence materially the properties of the compound, in which they exist.

In the conversion of cast into malleable iron, by the process of blooming, a considerable quantity of glass separates, which, as far as I have been able to determine, from a coarse examination, is principally silex, alumine, and lime, vitrified with oxide of iron.

Cast iron from a particular spot will make only cold short iron; whilst, from another spot, it will make hot short; but by a combination of the two in due proportions, good iron is produced; may not this be owing to the circumstance of their containing different metals of the earths, which in compound alloy, may be more oxidable than in simple alloys, and may be more easily separated by combustion?

Copper, M. Berzelius informs me, is hardened by silicium. In some experiments that I made on the action of potassium and iron on silex, the iron, as I have mentioned before, was rendered white, and very hard and brittle, but it did not seem to be more oxidable. Researches upon this subject do not appear unworthy of pursuit, and they may possibly tend to improve some of our most important manufactures, and give new instruments to the useful arts.

V. *Some Considerations of Theory illustrated by new Facts.*

HYDROGENE is the body which combines with the largest proportion of oxygene, and yet it forms with it a neutral compound. This on the hypothesis of electrical energy would shew, that it must be much more highly positive than any other substance; and therefore, if it be an oxide, it is not likely that it should be deprived of oxygene by any simple chemical attractions. The fact of its forming a substance approaching to an acid in its nature, when combined with a metallic substance, tellurium, is opposed to the idea of its being a gaseous metal, and perhaps to the idea that it is simple, or that it exists in its common form in the amalgam of ammonium. The phænomena presented by sulphuretted hydrogene, are of the same kind, and lead to similar conclusions.

Muriatic acid gas, as I have shewn, and as is further proved by the researches of MM. GAY LUSSAC and THENARD, is a compound of a body unknown in a separate state, and water. The water, I believe, cannot be decompounded, unless a new combination is formed; thus it is not changed by charcoal ignited in the gas by VOLTAIC electricity; but it is decompounded by all the metals; and in these cases hydrogene is elicited, in a manner similar to that in which one metal is precipitated by another; the oxygene being found in the new compound. This at first view, might be supposed in favour of the idea, that hydrogene is a simple substance; but the same reasoning may be applied to a protoxide as to a metal; and in the case of the nitromuriatic acid, when the nitrous acid is decomposed to assist in the formation of a metallic muriate, the body disengaged (nitrous gas,) is known to be in a high state of oxygenation.

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That nitrogene is not a metal in the form of gas, is almost demonstrated by the nature of the fusible substance from ammonia, and (even supposing no reference to be made to the experiments detailed in this paper,) the general analogy of chemistry would lead to the notion of its being compounded.

Should it be established by future researches that hydrogene is a protoxide of ammonium, ammonia a deutoxide, and nitrogene a tritoxide of the same metal, the theory of chemistry would attain a happy simplicity, and the existing arrangements would harmonize with all the new facts. The class of pure inflammable bases, would be *metals* capable of alloying with each other, and of combining with protoxides. Some of the bases would be known only in combination, those of sulphur, phosphorus,* and of the boracic, fluoric, and muriatic acids; but the relations of their compounds would lead to the

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* The electrization of sulphur and phosphorus, goes far to prove that they contain combined hydrogene. From the phænomena of the action of potassium upon them in my first experiments, I conceived that they contained oxygene, though as I have stated in the appendix to the last Bakerian lecture, the effects may be explained on a different supposition. The vividness of the ignition in the process, appeared an evidence in favour of their containing oxygene, till I discovered that similar phænomena were produced by the combination of arsenic and tellurium with potassium. In some late experiments on the action of potassium on sulphur and phosphorus, and on sulphuretted hydrogene, and on phosphuretted hydrogene, I find that the phænomena differ very much according to the circumstances of the experiment, and in some instances, I have obtained a larger volume of gas from potassium after it had been exposed to the action of certain of these bodies, than it would have given alone. These experiments are still in progress, and I shall soon lay an account of them before the Society. The idea of the existence of oxygene in sulphur and phosphorus, is however still supported by various analogies. Their being nonconductors of electricity is one argument in favour of this. Potassium and sodium I find when heated in hydrogene, mixed with a small quantity of atmospheric air, absorb both oxygen and hydrogene, and become nonconducting inflammable bodies analogous to resinous and oily substances.

suspicion of their being metallic. The salifiable bases might be considered either as protoxides, deutoxides, or tritoxides : and the general relations of salifiable matter, to acid matter, might be supposed capable of being ascertained by their relations to oxygene, or by the peculiar state of their electrical energy.

The whole tenour of the antiphlogistic doctrines, necessarily points to such an order ; but in considering the facts under other points of view, solutions may be found, which if not so simple, account for the phænomena, with at least equal facility.

If hydrogene, according to an hypothesis to which I have often referred, be considered as the principle which gives inflammability, and as the cause of metallization, then our list of simple substances will include oxygene, hydrogene, and unknown bases only ; metals and inflammable solids will be compounds of these bases, with hydrogene ; the earths, the fixed alkalies, metallic oxides, and the common acids, will be compounds of the same bases, with water.

The strongest arguments in favour of this notion, in addition to those I have before stated, which at present occur to me, are, First. The properties which seem to be inherent in certain bodies, and which are either developed or concealed, according to the nature of their combinations. Thus sulphur, when it is dissolved in water either in combination with hydrogene or oxygene, uniformly manifests acid properties ; and the same quantity of sulphur, whether in combination with hydrogene, whether in its simple form, or in combination with one proportion of oxygene, or a double proportion, from my experiments seems to combine with the same quantity of alkali. Tellurium, whether in the state of oxide or of hydruret,

seems to have the same tendency of combination with alkali ;
and the alkaline metals, and the acidifiable bases, act with the
greatest energy on each other.

Second. The facility with which metallic substances are
revived, in cases in which hydrogene is present. I placed
two platina wires, positively and negatively electrified from
500 double plates of 6 inches, in fused litharge ; there was
an effervescence at the positive side, and a black matter
separated at the negative side, but no lead was produced ;
though when litharge moistened with water was employed,
or a solution of lead, the metal rapidly formed ; the difference
of conducting power may be supposed to produce some diffe-
rence of effect, yet the experiment is favourable to the idea,
that the presence of hydrogene, is essential to the production
of the metal.

Third. Oxygene and hydrogene are bodies that in all cases
seem to neutralize each other, and therefore in the products
of combustion, it might be expected that the natural energies of
the bases would be most distinctly displayed, which is the case ;
and in oxymuriatic acid, the acid energy seems to be blunted
by oxygene, and is restored by the addition of hydrogene.

In the action of potassium and sodium upon ammonia,
though the quantity of hydrogene evolved in my experiments
is not exactly the same, as that produced by their action upon
water ; yet it is probable that this is caused by the imperfection
of the process ;* and supposing potassium and sodium to pro-

* There seems to be always the same proportion between the quantity of ammonia
which disappears, and the quantity of hydrogene evolved ; i. e. whenever the metals of
the alkalies act upon ammonia, (supposing this body to be composed of 3 hydrogene,
and 1 of nitrogene, in volume, 2 of hydrogene and 1 of nitrogene remain in combi-

duce the same quantity of hydrogene from ammonia and water, the circumstance at first view, may be conceived favourable to the notion, that they contain hydrogene, which under common circumstances of combination, will be repellent to matter of the same kind; but this is a superficial consideration of the subject, and the conclusion cannot be admitted; for on the idea that in compounds containing gaseous matter, and perhaps compounds in general, the elements are combined in uniform proportions; then whenever bodies known to contain hydrogene, are decomposed by a metal, the quantities of hydrogene ought to be the same, or multiples of each other. Thus in the decomposition of ammonia by potassium and sodium, two of hydrogene, and one of nitrogene remain in combination, and one of hydrogene is given off; and in the action of water on potassium to form potash, the same quantity of hydrogene ought to be expelled. From my analysis* of sulphuretted hydrogene, it

nation, and 1 of hydrogene is set free. And it may be adduced as a strong argument in favour of the theory of definite proportions, that the quantity of the metals of the alkalies and nitrogene, in the fusible results, are in the same proportions as those in which they exist in the alkaline nitrates.

* The composition may be deduced from the experiments in the last Bakerian lecture, which shew that it contains a volume of hydrogene equal to its own. If its specific gravity be taken as 35 grains, for 100 cubical inches, then it will consist of 2.27 of hydrogene, and 32.73 of sulphur. When sulphuretted hydrogene is decomposed by common electricity, in very refined experiments, there is a slight diminution of volume, and the precipitated sulphur has a whitish tint, and probably contains a minute quantity of hydrogene. When it is decomposed by VOLTAIC sparks, the sulphur is precipitated in its common form, and there is no change of volume; in the last case the sulphur is probably ignited at the moment of its production. In some experiments lately made in the laboratory of the Royal Institution, on arseniuretted and phosphuretted hydrogene, it was found that when these gasses were decomposed by electricity, there was no change in their volumes; but neither the arsenic nor the phosphorus seemed to be thrown down in their common states; the phosphorus was

would appear, that if potassium in forming a combination with this substance sets free hydrogene, it will be nearly the same quantity as it would cause to be evolved from water. And if the analysis of Mr. Proust and Mr. Hatchett, of the sulphuret of iron, be made a basis of calculation, iron, in attracting sulphur from sulphuretted hydrogene, will liberate the same proportion of hydrogene as during its solution in diluted sulphuric acid; and taking Mr. Dalton's law of proportion, the case will be similar with respect to other metals: and if such reasoning were to be adopted, as that metals are proved to be compounds of hydrogene, because in acting upon different combinations containing hydrogene, they produce the evolution of equal proportions of this gas, then it might be proved that almost any kind of matter is contained in any other. The same quantity of potash, in acting upon either muriate, sulphate, or nitrate of magnesia, will precipitate equal quantities of magnesia; but it would be absurd to infer from this, that potash contained magnesia, as one of its elements; the power of repelling one kind of matter, and of attracting another kind, must be equally definite, and governed by the same circumstances.

Potassium, sodium, iron, mercury, and all metals that I have experimented upon, in acting upon muriatic acid gas, evolve

dark coloured, and the arsenic appeared as a brown powder, both were probably hydrurets; this is confirmed likewise by the action of potassium upon arseniuretted, and phosphuretted hydrogene; when the metal is in smaller quantity than is sufficient to decompose the whole of the gasses, there is always an expansion of volume; so that arseniuretted and phosphuretted hydrogene contain in equal volumes, more hydrogene than sulphuretted hydrogene, probably half as much more, or twice as much more. From some experiments made on the weights of phosphuretted and arseniuretted hydrogene, it would appear, that 100 cubic inches of the first weigh about 10 grains, at the mean temperature and pressure, and 100 of the second about 15 grains.

the same quantity of hydrogene, and all form dry muriates; so that any theory of metallization, applicable to potash and soda, must likewise apply to the common metallic oxides. If we assume the existence of water in the potash, formed in muriatic acid gas, we must likewise infer its existence in the oxides of iron and mercury, produced in similar operations.

The solution of the general question concerning the presence of hydrogene in all inflammable bodies, will undoubtedly be influenced by the decision upon the nature of the amalgam from ammonia and a matter of so much importance ought not to be hastily decided upon. The difficulty of finding any multiple of the quantity of oxygene, which may be supposed to exist in hydrogene, that might be applied to explain the composition of nitrogene from the same basis, is undoubtedly against the simplest view of the subject. But still the phlogistic explanation, that the metal of ammonia is merely a compound of hydrogene and nitrogene; or that a substance which is metallic, can be composed from substances not in their own nature metallic, is equally opposed to the general tenour of our chemical reasonings.

I shall not at present, occupy the time of the Society, by entering any further into these discussions; hypothesis can scarcely be considered as of any value, except as leading to new experiments; and the objects in the novel field of electrochemical research, have not been sufficiently examined, to enable to decide upon their nature, and their relations, or to form any general theory concerning them, which is likely to be permanent.

Explanation of the Figures.

Fig. 1. The apparatus for electrizing potassium in gasses. A the glass tube. B the wire negatively electrified. C and D the cup and wire positively electrified.

Fig. 2. The apparatus for decomposing water, out of the contact of air, page 41. A A the cones containing the water. B B B the tubes for conveying the gas. C and D the pneumatic apparatus.

Fig. 3. The apparatus for decomposing, and recomposing water under oil. CC the wires for communicating the Voltaic electricity. DD the wires for producing the explosion. B the tube. A the vessel containing it. *a. d. c.* the level of the different fluids.

Fig. 4. The apparatus for exposing water to the action of ignited potash and charcoal, out of the contact of air. A the tube for water. B the iron tube. C the receiver for the ammonia. D the pneumatic apparatus.

Fig. 5. The apparatus for the decomposition of ammonia.

Fig. 6. A Voltaic apparatus, being one of the 200 which compose the new Voltaic battery of the Royal Institution. For the construction of this battery, and of other instruments applicable to new researches, a fund of upwards £1000. has been raised by subscription, from members of the Royal Institution. As yet, the whole combination has not been put into action ; but reasoning from the effects of that part of it which have been used, some important phænomena may be expected, from so great an accumulation of electrical power.
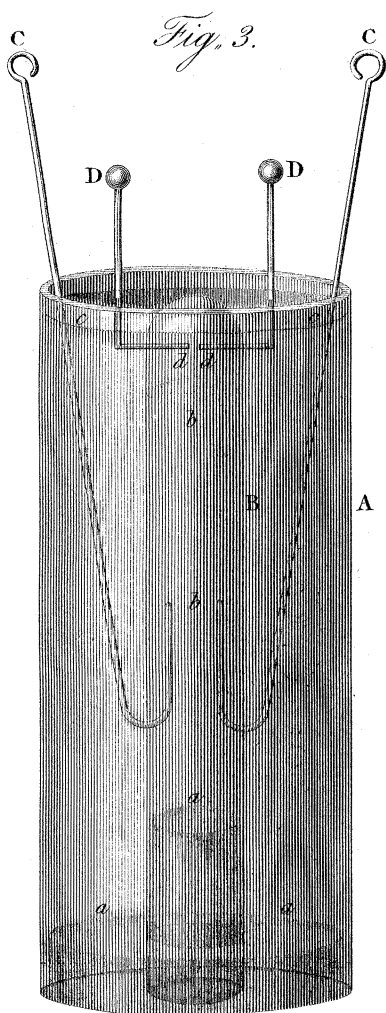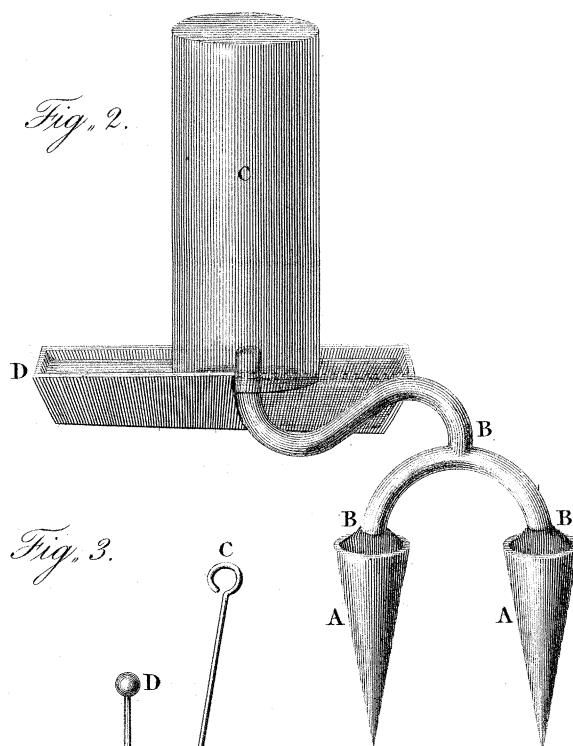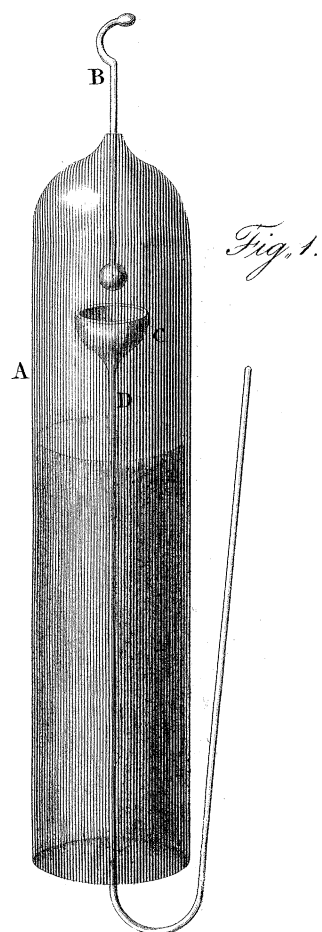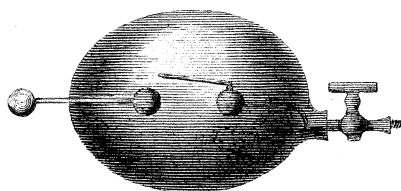
B

Fig. 2.

C

Fig. 1.

D

A

C

Fig. 3.

C

B

B

B

A

A

D

D

D

C

C

D

D

b

B

A

b

a

a

a

Fig. 5.

Fig. 4.

A

B

C

D

Fig. 6.

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